Author’s response to reviews

Title: Demographic, Psychosocial and Clinical Factors Associated with Postpartum Depression in Kenyan Women

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Response Letter to Reviewer Comments; Demographic, Psychosocial and Clinical Factors Associated with Postpartum Depression in Kenyan Women

Huynhnhu Le

1. The rationale for Kenya as a country to conduct this important research needs to be strengthened. It is not clear why Kenya was singled out as the country of interest to conduct this work. The authors hint at this when mentioning that the estimates are wide ranging and may be higher in a low resource context, and provide some research for the consequences associated with perinatal depression and prevalence in Kenya. In particular, the cultural context of Kenya is not mentioned, which would lend more credibility to the rationale for conducting this study. For example, Wittkowski, Gardner, Bunton, and Edge (2013; Journal of Affective Disorders) provides a mixed methods review of the risk factors of postpartum depression in sub-Saharan African that includes some of the culturally specific contexts.

We have strengthened this rationale by including the statement below and citing the paper mentioned above. The introduction section page 5 line number 82-86 now reads:
Identifying risk factors predisposing to postpartum depression is important especially in guiding proper screening for the condition. The authors have done substantial clinical work and research on depression in Kenya, and have identified the need for further assessment of risk factors for PPD in this population. Moreover, research has shown that postpartum depression risk factors in low income settings are significantly influenced by culture. This work is a step towards understanding these risk factors in the cultural context of Kenya.

2. The authors mention that they had conducted a study reporting the bivariate associations between postpartum depression and risk factors. Please mention the findings, and whether these are consistent with the current study's findings.

Thank you for pointing this out. In the referenced previous work, we only considered univariate associations with PPD. We have corrected this in the introduction page 5 line 92-94 to now state:

In a prior publication we reported univariate associations between postpartum depression and a broad spectrum of antepartum and postpartum risk factors. In this study, we expand on the previous work by assessing multivariable associations with PPD.

3. The study's goal was to estimate the prevalence and incidence of postpartum depression in Nairobi. It should be made clear that given that the authors did not conduct a diagnostic interview, the prevalence is related to significant postpartum depressive symptoms.

This is true, thank you for pointing this out. We have corrected this in the introduction section page 6 line 95 & 96 to read the prevalence is related to significant postpartum depressive symptoms.

4. What were the reasons for the 17 women who were lost to follow up after the antenatal assessment?

We have included the reason for loss to follow up in the results section page 9 line 173: ..10 of whom reported to have moved to their rural home and hence not willing to continue as study participants, the rest could not be traced on phone despite 3 attempts to reach them.
5. Additional psychometric information for the EPDS if used in Kenya and/or sub-Saharan Africa is warranted. It's unclear if the EPDS has been used in research in Kenya previously, and whether the cut-off scores have been validated for this country.

The EPDS had not been validated in Kenya until recently, we have referenced this new Kenyan study. The Limitations section now on page 14 line number 291 reads “While we established our EPSD cut off value for postpartum depression at > 10 in 2014 based on results of prior validation work, a recently published study in Kenya found the EPDS to be valid and reliable for identifying postpartum depression with an optimal sensitivity of 0.70 and a specificity of 0.72 at the cut off value of > 13 [34]. In our study, applying this cutoff value would have reduced the prevalence estimate to 12.28%, with only 21 mothers at or below the new cutoff value.”

6. Please report whether the findings differed significantly or not between the women with low and high PPD levels (tables 1-3).

Assuming the reviewer meant to report t-test or Chi-square test p-values for differences in the risk factors by PPD (Yes vs No), then we have added a symbol (¥) in the Tables to indicate which risk factors had a statistically significant difference by PPD.

Additionally we have added in the results section page 10 line number 196-198:

Among all the risk factors assessed in Tables 1 – 3, only number of children, antepartum depression, economic stress and conflict with partner had statistically significant differences by PPD status.

7. The incidence of postpartum depression symptoms is low in this study, and should be written as a %.

We now report the cumulative incidence (Number of incident cases/Number of women at risk) rather than the more difficult-to-interpret incidence density based on person-time of observation.

Page 9 results section in line 178 now reads:

The cumulative incidence of postpartum depression (PPD) among the 140 women who did not have antenatal depression was 21 new cases or 15.0% (95% CI: 0.10-0.22).
8. Discussion - 2nd paragraph: given the non-significant findings of the risk factors with relationships with mothers-in-law and partner - the conclusions are tenuous at best and should be further tempered. Is this potentially a culturally relevant risk factor to attend to for Kenyan mothers?

Relationship with mother-in-law is culturally relevant in Kenya more so in rural populations, where the mother in law is a key decision maker and would reside in the same compound as the mother, our study however focused on an urban population perhaps explaining our non-significant findings.

We have revised this section by deleting the phrase: “Though not statistically significant, our results suggest that mothers who have a good relationship with the mother-in-law and have a partner who takes part in cooking, cleaning, and childcare may be at lower depression risk.”

Instead the discussion section on page 12 (line number 243-247) now reads: “In addition to partner conflict, we asked women to evaluate the relationship with their mother in law. While we did not find a statistically significant association between postpartum depression and relationship with mother-in-law, this may be explained by the fact that our study focused on an urban population cohort. Urban settings differ from rural settings where communal living that includes the mother in law is the norm.”

We have further removed statements referencing mother in law in the subsequent sentences about the importance of good interpersonal relationships.

9. In the discussion, the range in rates of PPD differed from that in the introduction (8.3 to 39%, vs. 6 to 39%). Could the authors discuss why there was such a wide range. The authors also have data for prenatal depression that should be calculated in the results section, as they discuss this in the discussion (line 44).

We have included this statement and a reference to explain the wide range in the introduction section page 4 (line 55) reads:
This wide range has been largely attributed to difference in assessment tools as well as study populations across the different studies.

Table 2 shows the distribution of prenatal depression by PPD. We have revised the sentence in the discussion section page 13 line number 264 to only refer to PPD, the outcome of interest.

10. Minor:

a. The prevalence rates for depression in Kenya are reported under the risk factors section.

More than report prevalence we aimed to highlight the high rates found in; HIV populations, mothers of malnourished babies and pregnant adolescents, hence pointing out these risk factors.

b. "Post-partum" should be rewritten to postpartum. We have revised this to read postpartum throughout the manuscript.

Benedict Weobong (Reviewer 2): Demographic, Psychosocial and Clinical Factors Associated with Postpartum Depression in Kenyan Women.

General comments: 1

The paper may be considered for publication as it:

" Addresses and contributes to a mental health condition of public health importance that requires more research evidence to unpack its aetiology.

" Contributes to narrowing the mental health research-specific publication deficit in under-resourced regions of the world.

General comments: 2: Minor Essential Revisions

" The authors are encouraged to follow the STROBE-cohort studies guidelines in their report as this will help improve the structure/content of the paper and help with future systematic reviews/meta-analysis in this area of perinatal mental health.
Specific comments:

This paper could be improved if the following are addressed:

Abstract: Minor Essential Revisions

" Under results, it is inappropriate to use terminology like 'increased risk' when reporting odds ratios! And OR of 3.37 does not mean 'risk' is increased by 3-fold! It's also clear that the increased odds of postnatal depression due to antenatal depression exposure is not statistically significant, so wonder why the authors report it as if it were!

" The conclusion should be modified in light of the comment above.

Thank you for highlighting this. We agree, odds are not the same as risk, unless we assume depression is a rare outcome. We have revised the Results section in the abstract on page 2 (line 36) to now read:

“In multivariate analyses, the odds of having postpartum depression was increased more than three-fold in the presence of antepartum depression (OR=3.37, 95% CI: 0.98-11.64) and more than seven-fold in the presence of conflict with partner (OR=7.52, 95% CI: 2.65-23.13). However, confidence intervals around the estimates were wide.

We wanted to highlight the magnitude of association here, without ignoring the uncertainty around this estimate. This is why we also included the confidence intervals in our estimates.

Introduction: Minor Essential Revisions

" Lines 23-25: whilst I agree there's still some work to do re the evidence-base from appropriately designed cohort studies within SSA, I think there are quite some credible large cohort studies that have investigated risk factors for perinatal depression in the sub-region and these should be acknowledged. I can think of Weobong et al work which is perhaps the largest perinatal cohort study in Africa and the world!. Secondly, it appears your references are off as those references listed to support this argument relate to non-SSA studies! Indeed, I'm a bit concerned that your reference list is replete with references of studies largely conducted in other regions of the world when there are credible studies conducted in SSA that could be more appropriately cited!

We have included this statement and cited Weobong et al study in the introduction section page 4 line number 68 :A large longitudinal study in sub Saharan found a 50% increase in newborn illnesses.

We have further added in the discussion section on page 12 line number 237 a different publication from same authors (the Don Population Based Cohort Study) that states: One large cohort study in Ghana similarly reported antenatal depression as the strongest determinant of postnatal depression.
Methods:

General comments: Minor Essential Revisions

" Lines 41-53: this whole section should go under the results section.

This section has now been moved to the results section on page 9. (Line 169)

" It's not clear if the EPDS is validated for use in this (or similar) setting, and the reference provided is clearly inappropriate. Indeed, it does not appear it has been validated as the authors have used the original development/validation cut-offs. This could introduce measurement bias and I hope this is addressed in the discussion.

At the time of the study the EPDS had not been validated in this setting. We have cited the newly published study and validation cut offs recommended by the new study on page 14 the limitation section line number 291.

….a recently published study in Kenya found the EPDS to be valid and reliable for identifying postpartum depression with an optimal sensitivity of 0.70 and a specificity of 0.72 at the cut off value of > 13

" Also it is not clear how participants were selected and if this was random. I understand this analysis cohort was taken from another study with a different goal, but given this is an epidemiological investigation, issues that may affect internal/external validity of the findings are important and adequate information provided. It's however refreshing to note that there were very few refusals and participants were largely comparable in terms of baseline socio-demographic factors.

We have stated in our methods section page 6 line number 112…following registration at the MCH clinic, trained study nurses informed all attending women in their third trimester of the ongoing study. After giving an explanation of the study, a written informed consent was obtained from those who met the inclusion criteria.

" Sample size calculation: this is not discussed but think it should! What were the assumptions and how powered was this analysis (compared to previous studies)?

We have now included in the methods section on page 8 (line 154) the statement:

For this analysis, we did not have a primary exposure of interest since our goal was to assess multiple risk factors. However, if we consider the exposure of antenatal depression, we had 72% power to detect a crude odds ratio for prenatal depression of 3.1 comparing women who had PPD and those who did not.
Data analysis:

General comment: Minor Essential Revisions

" Not sure if potential moderation effects were explored (as not mentioned/reported); could there be interaction effects? And were these explored? If not why?

Great point, we did assess effect modification (EM) but found no evidence of interactions. We looked at the interactions between:

   a. antepartum depression and partner helping with child care

   b. antepartum depression and relationship with partner's mother

We did not feel strongly about effect modification by the factors we assessed. Therefore, our assessment of EM was only exploratory and not as part of the original analysis plan.

Results:

General comments: Minor Essential Revisions

" Authors are strongly encouraged to not over-state some of the findings. I don't see an association with: mother in-law, helpful partner. Nor, vaginal delivery/nursery admission as the OR CIs include 1! I assume the authors may have claimed an association based on the p-value but this is not to be encouraged as we all know the p-value is very problematic and may not be used alone without the CIs.

Similar to the comment above regarding our results, we report both point estimates and 95% CIs to show the level of uncertainty in our estimates and whether the CIs include 1.
Discussion:

General comments: Minor Essential Revisions

"As commented on already, the authors may have totally ignored the relatively large and credible body of evidence re determinants of perinatal depression in SSA and this is worrying. E.g. Weobong et al found very strong and large associations between biological factors/birth-related factors and postnatal depression! This was not replicated in this study and would be useful to learn why this might have been the case in Kenyan women. The authors have made quite strong arguments for the mental health needs of women to be given proper recognition, based on this piece of work. I am not certain that this report provides the appropriate platform to make such recommendations, and I see this to be quite inappropriate. A cross-sectional design provides exploratory, hypothesis generating findings necessary for a more analytical/definitive design to be commissioned. We do not know if any of the findings here are causal and therefore could not use this as a basis to make strong arguments for interventions as proffered in this report. Indeed, the correlations reported here are largely very weak and this calls for a cautious interpretation of the findings. The authors should be discussing their findings in the context of these important considerations, advancing potential strategies for collecting prospective data in order to appropriately answer the questions set out in this report.

"The rather odd finding of no statistically significant association between antenatal depression and postnatal depression in this study should be discussed. Antenatal depression is perhaps the most consistent independent predictor of postnatal depression and the inability to replicate this in this study warrants some discussion.

We have cited Weobang’s study. We have also addressed in the limitation section on page 14 line number 282 that the lack of association with certain factors may stem from our small sample size.

Our cohort study found a strong association between postnatal and antenatal depression, as well as with two other factors measured in the antenatal period, conflict with partner, and social support. We have removed the recommendation suggested for interventions like IPT as the study did not address this.

We have included a recommendation for future studies to focus on larger population-based longitudinal study designs in our conclusion section page 15 (line 309).
This now reads: “Future multi-wave cohort studies with larger population samples are recommended.”

Our study did indeed find a statistically significant association between antenatal depression and postnatal depression. This is indicated both in our results and discussion section on page 12 (line 235)